



Agricultural policy evaluation with large-scale observational farm data: Environmental impacts of agri-environmental schemes

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Agricultural policy evaluation with large-scale observational farm data: Environmental impacts of agri-environmental schemes

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Abstract

Agri-environmental schemes (AES) target at improving environmental status of cultivated land by remunerating farmers willing to commit to higher environmental standards. Thus far, no consensus exists whether AES incentivize adoption of pro-environmental production or simply offer windfall profits for those already operating at lower intensities. Using farm-level data, evaluation typically rests on comparing farms with and without AES. For differencing out unobservables that drive farmers into AES participation and therefore confound impact measurement, DID-matching methods are widespread, yet critical reflection remains sparse. We target at closing this gap by shedding light on the implicit assumptions about cause and effect paths linking participation and treatment outcome. We discuss challenges for identification of causal effects in presence of unobservable confounders over a broad range of methods and illustrate DID methods to estimate AES effects on land-use in West Germany.

Keywords: policy impact evaluation, agri-environmental schemes, DID-matching, kernel matching

JEL codes: Q18, Q24, Q58, D04

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Table of contents

Abstract	i
1 Introduction	1
2 Evaluation of agri-environmental policy within the EU's CAP	3
3 Effects of interest, potential impact pathways and identification.....	5
3.1 The causal model and effects of interest	5
3.2 Selection, impact pathways and identification strategies	6
4 Techniques for identification strategies.....	8
4.1 Matching.....	8
4.2 Differences-in-differences	10
4.3 DID-Matching	12
5 Illustration: Evaluating AES in Germany.....	14
5.1 Data, sample and AES participation.....	14
5.2 Average treatment effects of AES participation.....	18
5.3 Average treatment effects of AES participation by farm types.....	22
6 Discussion, future research and policy implications	24
References	25
Appendix	30

List of tables

Table I: Means of farm characteristics by treatment state and cohort	16
Table II: Coefficients of logit model for AES uptake by participation cohort	17
Table III: DID-matching outcomes for fertilizer expenditures	19
Table IV: DID-matching outcomes for fertilizer expenditures, reduced imbalance	22
Table V: DID-matching outcomes for livestock farms	23
Table AI: AES uptake by year in West German.....	30

List of figures

Figure 1: Possible cause-and-effect paths.....	7
Figure 2: Matching, conditioning on S (a) and X and Y_2 (b).....	9
Figure 3: Average treatment effect from DID	12
Figure 4: DID-Matching (a) and DID-Matching on the pre-treatment outcome (b)	13
Figure 5: Covariate balance before and after matching (outcome fertilizer expenditure)	20
Figure 6: Improved covariate balance (added exact matching on share of rented land)	21
Figure A1: Covariate balance for outcome grassland share	31
Figure A2: Covariate balance for livestock farms	32

1 Introduction

Agri-environmental schemes (AES) together with the 2013 introduced ecological focus areas are the most important policy instruments for the improvement of sustainable agricultural practices within the EU's Common Agricultural Policy (CAP). While both measures are on voluntary base, ecological focus areas offer farms higher payments for additional efforts in more sustainable land use, for instance extensively managed buffer strips. Within AES – mandatory in the rural development plans of the EU Member states since 1992 – farmers commit to produce at higher ecological standards such as extensive crop production for five years contract periods. In return, farms receive compensation payments for higher production cost and potential yield losses.

Despite the availability of a large variety of quasi-experimental methods for causal impact evaluation (Imbens & Rubin, 2015), empirical evidence on AES contribution to environment states of cultivated land is limited (Yoder, Ward, Dalrymple, Spak, & Lave, 2019). Only few studies tempt to evaluate agri-environmental policy based on causal impact estimators, although the necessary large-scale farm-level data sets such as the Farm Accountancy Data Network (FADN) offer a strong base. Consensus persists on the core challenge for causal impact analysis: the non-experimental conditions and data unavailability on farmers preferences (D'Alberto, Zavalloni, Raggi, & Viaggi, 2018). Since AES adoption is voluntary, and driven by environmental preferences and pro-environmental behavior of the manager, such unobservable characteristics can confound identification of causal effects. In presence of this selection bias, a direct comparison of participating with non-participating farms will not represent the true causal impact of the policy. Hence, relying on existing large-scale data sets, the central question is how to overcome this selection bias.

To address the selection bias, several quasi-experimental methods exist: by ensuring comparability of farms and accounting for self-selectivity issues, these methods offer estimates that can be interpreted as unbiased causal effects of AES participation on the environmental state of the farmed land. Typically, based on a set of farm characteristics, compared farms differ then only in their participation state. Remaining differences in the outcome of interest such as land use intensity can then be traced back to AES participation. Many of these farm characteristics are, however, unobserved or are genuinely unobservable. Exploiting the advantage of panel data, available estimation techniques difference out potentially confounding unobservables by difference-in-difference (DID) estimators combined with matching (Heckman, Ichimura, & Todd, 1997).

Identification of these causal effects, however, relies on specific assumptions about how the set of farm characteristics is related to the participation decision and outcome (Elwert & Winship, 2014). First, unbiased estimation based on DID-matching requires that the unobserved characteristics (e.g., environmental preferences) have a time invariant effect on the outcome. Second, conditional on a set of observable characteristics parallel outcome trends must exist. The latter assumption implies that participating farms with similar characteristics would have

experienced the same outcome trend as those farms in the control group in the absence of the policy. Robustness and validity of results hence strongly depend on whether these assumptions can be justified. A profound justification, in turn, requires an explicit representation of impact pathways (e.g. Petrick & Zier, 2011, for agricultural policy). Thus far, the impact pathways implicitly assumed by the chosen method are rarely made explicit in agricultural policy evaluation. This hinders discussion about generalizability of the results and bears danger of potentially biased estimations of the causal effect.

Against this backdrop, we target at investigating two major questions of agricultural policy evaluation: (i) how to deal with self-selection in the econometric model specification to estimate AES effects and (ii) how to use existing large-scale data such as the FADN effectively by addressing self-selection in the analysis and retrieve reliable policy outcome measures. To this end, we clarify how existing quasi-experimental evaluation methods perform under different impact pathways and illustrate potential biases that arise from deviations of the assumed impact pathway. Then, based on the theoretical representation of the cause and effect paths, we illustrate DID-estimators for evaluating AES for Germany in the period 2000-2006. We compare several DID-matching estimators using a range of available algorithms including kernel matching with optimal bandwidth selection procedures (Galdo, Smith, & Black, 2008; Huber, Lechner, & Steinmayr, 2015; Loader, 1999) and post-matching regression adjustment (Abadie & Imbens, 2006, 2011). We find that the kernel methods perform better in terms of efficiency and bias reduction than nearest neighbor estimators.

Thus, our study offers four contributions. First, we provide an updated review of the literature showing that available results on AES effects are ambiguous and partial at best. Second we are offering an explicit theoretical framework of the cause and effect paths of AES participation using path diagrams (Pearl, 1995). These serve as a basis for discussing the assumptions of different quasi-experimental methods in the context of agricultural policy evaluation and enable us to identify knowledge gaps about adoption behavior to be potentially addressed within future studies on AES adoption. Third, using FADN data for West Germany, we find that participation has led to notable increases in farms' grassland share and reduction in fertilizer intensity in the first program period, 2000-2006. Based on a unique farm type specific analysis, we find environmental effects for specialized livestock farms only. Fourth, in doing so, we present a data strategy that uses available large-scale data efficiently without additional experimental cost for evaluation purposes. Our results further reveal need to incorporate more meaningful outcome measures to be addressed in future design of monitoring of the CAP. As such, our illustration serves as an instructive case study for AES evaluation and can easily be transferred to other European countries and programming periods in order to enhance understanding of the impact of agri-environmental measures.

We start by reviewing the literature and discuss potential sources of biases (section 2). In section 3, we structure underlying assumptions of different identification strategies and discuss consequences of possible derivations from the theorized causal relationships by using path diagrams. We then discuss empirical strategies for identification based on these cause- and effect graphs: matching, DID and DID-matching (section 4). Then we illustrate the application

of DID-matching to evaluate AES participation in Germany (section 5). Finally, we discuss a future research agenda for experimental studies and policy implications in section 6.

2 Evaluation of agri-environmental policy within the EU's CAP

Evaluations of the causal impacts of pro agri-environmental agricultural practices are scarce as pointed out by Börner et al. (2017) and Yoder et al. (2019), who find in their extensive literature review 18 studies that quantify environmental outcomes of conservation practices adoption. For the specific environmental measures within the CAP, evaluation reports mainly rest on simple before and after comparisons of farms that are deemed comparable at the same point in time after the policy implementation (e.g. Hart, Mottershead, Tucker, Underwood, & Maréchal, 2017). However, in case of non-random participation decisions, participating farms differ from non-participating farms in core characteristics that are either important for the outcome of interest, such as the farms' production intensity, or for the participation decision, such as environmental preferences (Bartkowski & Bartke, 2018; Lastra-Bravo, Hubbard, Garrod, & Tolón-Becerra, 2015; Pascucci, de-Magistris, Dries, Adinolfi, & Capitanio, 2013; Zimmermann & Britz, 2016). For example, farms could participate in AES simply because they already operate at low intensities and therefore program requirements represent a low hanging fruit with remuneration (Defrancesco, Gatto, Runge, & Trestini, 2008). If low intensity farms self-select into the AES, a direct comparison between the outcomes of participating and non-participating farms would suggest an impact of the program on farming practices, whereas the difference simply could have been caused by the lower pre-participation intensities. Such potential situations challenge causal interpretation of effects since the payments for AES participation would merely generate windfall profits without any additional environmental effects (Chabé-Ferret & Subervie, 2013; Hynes & Garvey, 2009).

To our knowledge, only few studies tempt to estimate causal impacts of AES participation on the environment using quasi-experimental approaches: Arata and Sckokai (2016) for 5 selected EU member states, Chabé-Ferret and Subervie (2013) for France and Pufahl and Weiss (2009) for Germany. Other policy outcomes that have been examined are farm income (Udagawa, Hodge, & Reader, 2014) or farm productivity (Mennig & Sauer, 2019) or the environmental outcomes of organic farming (Cisilino, Bodini, & Zanoli, 2019). Among these only Chabé-Ferret and Subervie (2013) critically reflect selection bias and identification assumptions for the causal effect. Based on empirical tests and a detailed robustness analysis, these authors provide an assessment framework for France, which however, cannot easily be transferred given the limitations of other countries' data structure and availability.

Regarding our illustration region of West Germany, the environmental impact of AES has been evaluated using DID-matching by Pufahl and Weiss (2009) and Arata and Sckokai (2016), though with differing results. Pufahl and Weiss use a dataset that comprises a rich set of farms' bookkeeping data for the period 2000-2005 to evaluate the effects of AES participation on a number of outcomes, including grassland share and expenditures for pesticides and fertilizer. Based on their DID-Matching approach, they find an average treatment effect of the treated

(ATT) of reduction in fertilizer expenditure of 9.4% and 4.7% reduction in pesticide expenditure. Moreover, they find a positive causal effect of AES participation on farm's share of grassland of about 9%. They conclude that AES participation leads to the desired environmental effects of these programs. Arata and Sckokai (2016) use the European Farm Accountancy Data Network (FADN) to construct a balanced panel for the period of 2003 to 2006 and compare the outcomes over five EU-countries. For Germany, they find an ATT in fertilizer expenditure reduction of € 33/ha but only for a subsample in which the share of AES-payments on farm income is larger than 5%. They translate this reduction into a percentage reduction of 89%. In contrast to Pufahl and Weiss, Arata and Sckokai (2016) do not find significant effects of participation on pesticide expenditures and share of grassland. .

The reasons for the different outcomes in these studies can be manifold: Arata and Sckokai employ a small dataset for Germany (those that adopt AES in 2003), which restricts generalizability of their result. Pufahl and Weiss (2009) dispose of a much larger sample of treated farms; however, they have to exclude about 80% of treated farms in order to reduce covariate bias to acceptable terms. Moreover, northern German farms are overrepresented in their dataset.

Within our approach, we target at shedding light on the sensitivity of results to post matching covariate bias and on maximizing the matched pairs from the FADN sample of the whole implementation period 2000-2006. This way we ensure that results are representative, even allowing for separating impact effects across farm types. Using the EU FADN database, however, limits the choice of outcome measures. The majority of the aforementioned studies refer to input intensity of fertilizer and plant protection on cost basis and grassland share, since these represent implicitly environmental indicators simply because less input per land unit would do less harm to the environment. Some EU member states already collect data on environmental performance of the agricultural sector and several studies try to use national farm accountancy datasets to assess farm sustainability on different dimensions (Dabkienė, 2016; Kelly et al., 2018). For example Buckley, Wall, Moran, and Murphy (2015) derive farm gate balances of N and P from the Irish National Farm Survey. These farm gate nutrient-balances offer more detailed information on the environmental performance than fertilizer inputs on expenditure base. Additionally, recent studies combine farm accountancy data with routinely collected information from the Integrated Administration and Control System (IACS) and Land Parcel Information System (LPIS), but also with survey data (e.g. Leonhardt, Penker, & Salhofer, 2019) or with detailed information about scheme participation (Mennig & Sauer, 2019). Even combinations with large-scale data sets based on remote sensing and geospatial technologies have been proposed (e.g. Lynch, Donnellan, Finn, Dillon, & Ryan, 2019).

These examples show that combining routinely collected large-scale datasets offer promising possibilities to construct better outcome measures from ecological indicators. While these improved measures would be valuable, they do not solve the problem of confounding in the causal analysis nor the efficient use available data. Since one of our aims is to illustrate how available data can be employed more effectively for the purpose of agri-environmental policy evaluation, we focus on measures that are available within the FADN. By doing this, we deliver

insights about the usefulness of combining these large-scale bookkeeping datasets with more meaningful environmental indicators.

3 Effects of interest, potential impact pathways and identification

3.1 The causal model and effects of interest

We are interested in the causal effect of AES participation on an outcome of interest. For illustration, we refer to the potential outcome model (Rubin, 1974). Each farm has a potential outcome under each treatment state, that is, AES participation and non-participation. The individual causal effect is defined as the difference in the individual potential outcomes

$$\delta_i = y_i^1 - y_i^0,$$

with y_i^1 denoting the potential outcome of farm i if it belonged to the treated group and y_i^0 denoting the potential outcome of farm i if it belonged to the control group. This individual treatment effect is unknown, as only one of these two states is observable.

To get an estimate of the average treatment effect, the individual potential outcomes are treated as realizations of population-level potential outcome random variables Y^1 and Y^0 (Ho, Imai, King, & Stuart, 2007). For multiple observations, the average treatment effect (ATE) is defined as

$$ATE = \frac{1}{N} \sum_{i=1}^N y_i^1 - y_i^0 = E[Y^1 - Y^0] = E[Y^1] - E[Y^0], \quad (1)$$

where realized outcomes are observed under the treatment assignment mechanism $D \in \{0,1\}$ such that $E[Y^1|D=1]$ and $E[Y^0|D=0]$.

In the context of evaluating AES, the causal effect of interest is the average treatment effect of the treated (ATT), that is, how did AES contribute to increase the environmental indicator of those who participated. The ATT is defined as the difference between the observed outcome of the treated farms and the potential outcome of these farms if they had not participated in AES.

$$ATT = E[Y^1 - Y^0|D=1] = E[Y^1|D=1] - E[Y^0|D=1] \quad (2)$$

If those in the in the treatment group performed, on average, no better or worse in their counterfactual control state compared to those in the control group, the unobservable potential outcome of the treatment group $E[Y^0|D=1]$ can be substituted by the observed outcome of the control group $E[Y^0|D=0]$:

$$E[Y^0|D=1] = E[Y^0|D=0]. \quad (3)$$

Eq. (3) states the sufficient condition for identifying the average treatment effect of the treated. This condition allows estimating the causal effect as a contrast between observed means of the treatment and control groups (Athey & Imbens, 2017a; Holland, Glymour, & Granger, 1985):

$$ATT = E[Y^1|D = 1] - E[Y^0|D = 0] = E[Y^1] - E[Y^0]. \quad (4)$$

In case of randomization Y^1 and Y^0 are probabilistically independent of D and the condition for causal effect estimation as given in eq. (3) is satisfied.

In case of AES, condition (4) may be violated as farmers self-select into the agri-environmental program for instance based on preferences or low adjustment cost of the farm program after commitment to AES. If the independence of treatment assumption (eq. (3)) is violated, ATT will be biased from substituting $E[Y^0|D = 0]$ for $E[Y^0|D = 1]$ because $[Y^0|D = 1] \neq E[Y^0|D = 0]$. Several identification strategies exist on how the unobserved potential outcome can best be estimated (Angrist 1999), which we discuss next.

3.2 Selection, impact pathways and identification strategies

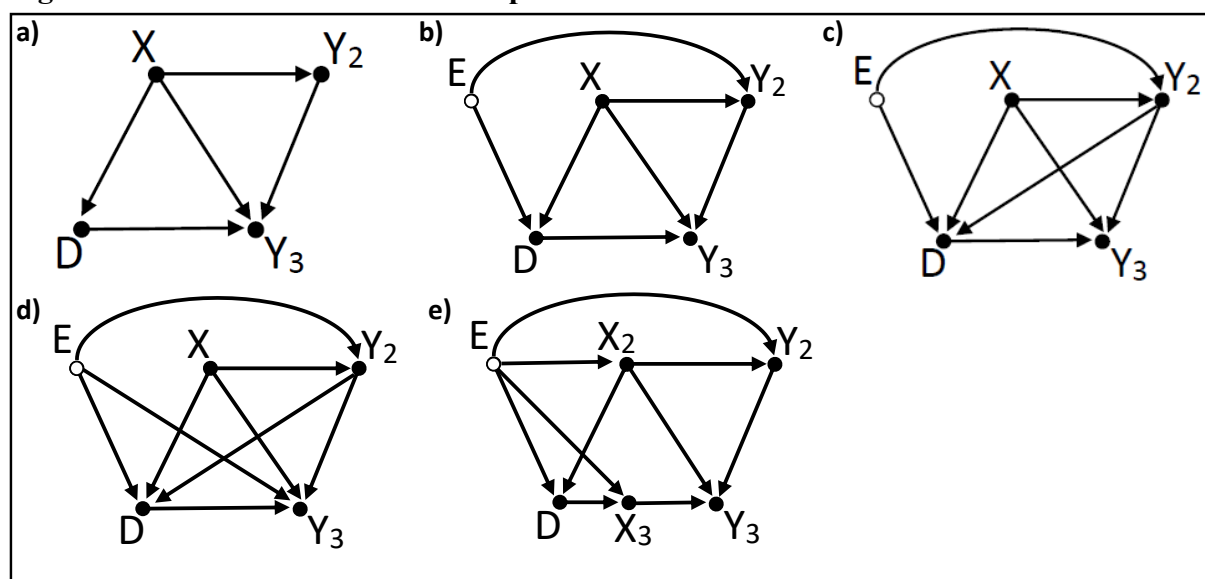
Identification strategies can be structured based on the presumed selection mechanism, that is, the uptake of pro-environmental farming under AES. We distinguish between three situations: i) selection on observables, e.g. matching techniques; ii) selection on unobservables, e.g. differences-in-differences (DID) approaches; iii) a mixture of both, e.g. DID-matching, which has been applied by most scientific AES evaluation studies. In what follows, we illustrate potential cause and effect paths and these strategies using directed acyclic graphs (Elwert & Winship, 2014).

In Figure 1 we present possible cause and effect paths, all of which imply different empirical strategies for causal analysis. Variable D represents participation, which occurs in period 2. Y_2 and Y_3 are the pre- and post-treatment outcomes, in our case the level of environmental performance. A set of observable characteristics, summarized in matrix X , such as the production portfolio, intensity and farm type. The average causal effects of AES participation (eqs. (1) and (2)) are identified when all non-causal paths between D and Y_3 are blocked by conditioning on a suitable set of observed variables (Pearl, 2000). As such, AES participation could be determined solely by how easy the program requirements can be fulfilled. This would be reflected in the pre-participation production program by e.g. capital intensity, crop rotation or livestock density included in the vector X (cf. Figure 1a). These observed characteristics then determine the treatment decision but also the pre-treatment outcome. The non-causal paths between D and Y_3 in Figure 1a are $D \leftarrow X \rightarrow Y_3$ and $D \leftarrow X \rightarrow Y_2 \rightarrow Y_3$. Conditioning on X blocks both backdoor paths and the causal effect is identified.

Alternatively, participation could additionally be determined by unobserved environmental preferences, E . An unobserved environmental preference leads to a better environmental performance before treatment for farms with otherwise similar characteristics (cf. Figure 1b). Two new backdoor paths arise: $D \leftarrow E \rightarrow Y_2 \rightarrow Y_3$ and $D \leftarrow E \rightarrow Y_2 \leftarrow X \rightarrow Y_3$. The first path is

blocked by conditioning on the pre-treatment outcome Y_2 . In the second path Y_2 is a collider variable that causes a spurious correlation between its two common causes E and X (e.g., Elwert & Winship, 2014). Conditioning on X and Y_2 blocks all backdoor paths in this situation. If now environmental preferences lead to better environmental performance before treatment and additionally this better performance makes program participation more attractive (above the effect of production intensity and production program captured in X) as illustrated in Figure 1, model c), then a new backdoor path arises, but it will be blocked by conditioning on Y_2 . Thus conditioning on X and Y_2 is consistent for causal models a) through c).

Figure 1: Possible cause-and-effect paths



Notes: a) selection on observables b) selection on unobservables (time constant) c) selection on unobservables (time constant) and on the pre-treatment outcome d) selection on unobservables (time constant and time variant) and on the pre-treatment outcome e) separate effect of unobservables on farm characteristics with post-treatment characteristics as mediating factor between treatment and outcome

Yet another possibility is that the unobserved time invariant effect of E on Y_2 might also affect the post-treatment outcome Y_3 separately, which would result in an unblocked path $D \leftarrow E \rightarrow Y_3$ as illustrated in Figure 1d. This is the case if unobserved pre-treatment characteristics are associated with the dynamics of the outcome variable but unbalanced between the treated and the untreated (Abadie, 2005). For example if farmers with strong (unobserved) environmental preferences already operate at low intensity levels for harmful inputs, for instance by choice of crops and plant protection (fixed effect), and because of these preferences these farms are likely to adjust the production such as to reduce the harmful environmental outcome over time (before treatment). Participation in AES and outcome after treatment will both be affected by the adjustment rate over time driven by preferences and ability. The unobserved environmental preferences could lead to a composed effect consisting of a time invariant and time variant component. Conditioning on the pre-treatment outcome would control for the time invariant part only and estimated treatment effects would be biased upwards.

Figure 1e explicitly depicts that the treatment effect of D on Y_3 is induced by a change in the land use practices that are reflected in the farm characteristics in the post-treatment period X_3 . If environmental preferences have a separate effect on the post-treatment farmland management, which in turn affects the outcome, we must exclude this influence from the treatment effect. This is because this part of the change is not induced by the treatment, but would have occurred also without the treatment, yet only for those with pro-environmental preferences. Hence, the backdoor-path $D \leftarrow E \rightarrow X_3 \rightarrow Y_3$ cannot be blocked as E is unobservable and conditioning on X_3 would lead to overcontrol bias (Elwert & Winship, 2014) as the participation state affects part of the change in X_3 .

Additionally, other sets of unobservable characteristics might also determine treatment selection and outcome analogous to e). This may include the propensity to adopt new technologies or prior adoption of pro-environmental management practices such as sustainable intensification strategies (Weltin et al., 2018). Prior experience with sustainable intensification (SI) strategies might also influence the decision to adopt AES as well as the environmental outcome. As the FADN does not contain a measure for this prior experience, the influence on the outcome of these SI strategies will be ascribed to the effect of AES, leading to an overestimation of the AES impact.

4 Techniques for identification strategies

The portfolio of available methods for estimation the causal effect under different potential sources of bias is large (Imbens & Rubin, 2015). We illustrate the core concepts of the most popular techniques for conditioning on the identified confounding characteristics, namely matching, difference-in-difference (DID) and DID-matching in the following.

4.1 Matching

Matching identifies the ATT under the assumptions of ignorable treatment assignment, overlap and stable unit treatment value (Imbens, 2004; Rosenbaum & Rubin, 1983; Rubin, 1980). Ignorability states that the potential outcomes are independent of treatment status given a vector of pre-treatment covariates \mathbf{S} ,

$$(Y^0, Y^1) \perp\!\!\!\perp D \mid \mathbf{S}.$$

If ignorability is valid, it implies the following assumptions:

Assumption 1: $E(Y^1 \mid D = 1, \mathbf{S}) = E(Y^1 \mid D = 0, \mathbf{S})$,

Assumption 2: $E(Y^0 \mid D = 1, \mathbf{S}) = E(Y^0 \mid D = 0, \mathbf{S})$,

which means that if the treated were untreated, they would have realized the same expected outcome (on average) as the untreated and vice versa. Under these assumptions, substituting

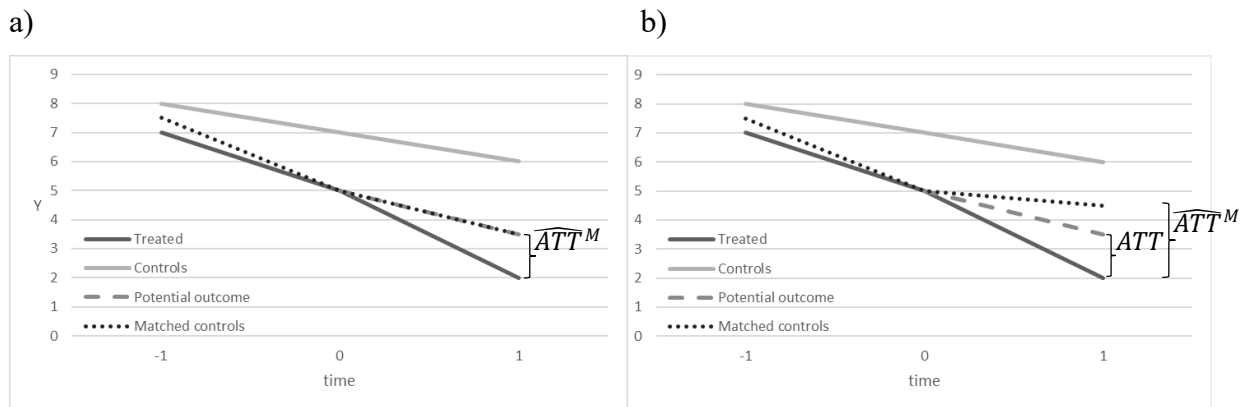
the unobserved potential outcome of the treated with the observed outcome of the control group (assumption 2) identifies the ATT:

$$\begin{aligned} ATT &= ATT^M = E(Y^1 - Y^0 | D = 1, \mathbf{S}) \\ &= E(Y^1 | D = 1, \mathbf{S}) - E(Y^0 | D = 1, \mathbf{S}) \\ &= E(Y^1 | D = 1, \mathbf{S}) - E(Y^0 | D = 0, \mathbf{S}) \end{aligned}$$

To ensure ignorability, all covariates that are related to the participation decision and the outcome must be included in \mathbf{S} (Athey & Imbens, 2017b; Stuart, 2010; VanderWeele & Shpitser, 2011).

This situation is illustrated in Figure 2. There are three points in time and treatment assignment occurs in $t = 0$. The outcome Y is an environmentally harmful output – i.e. the higher Y , the less environmentally sustainable is the production. Four outcome trends arise: for the treated (dark solid), the untreated (grey solid), the matched controls (dashed) and the potential outcome for the treated (dotted). If balancing on \mathbf{S} satisfies ignorability, the potential outcome trend of the treated coincides with the observed outcome of the matched control trend and \widehat{ATT}^M is consistent (Figure 2a). In this case, cross-sectional data would be sufficient to identify the causal effect. However, if instead of \mathbf{S} only a subset, \mathbf{X} , is observed, e.g. if an unobserved fixed effect like environmental preferences determines treatment and outcome, conditioning on \mathbf{X} fails to ensure ignorability. If we assume a cause and effect path as in Figure 1b) or c), we can block the influence of E by matching on \mathbf{X} and the pre-treatment outcome Y_2 , stating that $(Y^0, Y^1) \perp\!\!\!\perp D | \mathbf{X}, Y_2$, including $(Y^0, Y^1) \perp\!\!\!\perp E | \mathbf{X}, Y_2$. This equals to stating that differences in Y_3 of farms with the same levels of \mathbf{X} and Y_2 are strictly related to the treatment status and not caused by unobserved manager preferences. This requires full balance on Y_2 in the matched sample.

Figure 2: Matching, conditioning on \mathbf{S} (a) and \mathbf{X} and Y_2 (b)



If however environmental preferences have a separate effect on the post-treatment farm management and the corresponding farm characteristics as for the situation described in Figure 1d) or 1e), the outcome trend of the matched control farms could differ from the potential outcome trend of the treated. For example, although being similar in \mathbf{X} and Y_2 the controls

could lack an interest in adapting new land management practices to further reduce environmental harm. This would cause the outcome trend of the matched controls to be flatter than the potential outcome path, resulting in an overestimation of the impact of AES participation (\widehat{ATT}^M in Figure 2b).

ATT^M can be estimated with the sample equivalents of the expectancy values:

$$\widehat{ATT}^M = \frac{1}{N_1} \sum_{i=1}^{N_1} \hat{m}_1(\mathbf{S}_i) - \hat{m}_0(\mathbf{S}_i)$$

with $\hat{m}_1(\mathbf{S})$ and $\hat{m}_0(\mathbf{S})$ as estimators for the group specific expectancy values $E(Y|D = 1, \mathbf{S})$ and $E(Y|D = 0, \mathbf{S})$. \widehat{ATT}^M sets $\hat{m}_1(\mathbf{S})$ to the observed value Y_i^1 and $\hat{m}_0(\mathbf{S})$ to \tilde{Y}_i^0 , which is imputed from Y_i^0 through a distance function that is based on the vector \mathbf{S} (Cerulli, 2015).

4.2 Differences-in-differences

Differences-in-differences (DID) removes bias in the comparison of treated and control units that arise from permanent differences between these groups and from time trends unrelated to the treatment (Imbens & Wooldridge, 2009). In the DID framework, identification of the ATT is accomplished via the parallel trends assumption (PTA). Let $Y_{t=0}^1$ and $Y_{t=1}^1$ denote the pre- and post-intervention outcomes for the treated and $Y_{t=0}^0$ and $Y_{t=1}^0$ denote the pre- and post-intervention outcomes for the control group. In the potential outcome framework the ATT is defined as

$$ATT = E[Y_{t=1}^1 - Y_{t=1}^0 | D = 1]$$

To find an estimand for the unobservable potential outcome $E[Y_{t=1}^0 | D = 1]$, the population average difference in the control group is subtracted from the population average difference in the treated group:

$$\begin{aligned} ATT &= ATT^{DID} = E[Y_{t=1}^1 - Y_{t=1}^0 - Y_{t=0}^0 + Y_{t=0}^1 | D = 1] \\ &= E[Y_{t=1}^1 - Y_{t=0}^1 | D = 1] - E[Y_{t=1}^0 - Y_{t=0}^0 | D = 1] \end{aligned}$$

The PTA states that had the treated not been treated, their change over time would have equaled to the trend of the non-treated, which yields:

$$E[Y_{t=1}^1 - Y_{t=0}^1 | D = 1] = E[Y_{t=1}^0 - Y_{t=0}^0 | D = 0],$$

and can be substituted into ATT^{DID} :

$$ATT^{DID} = E[Y_{t=1}^1 - Y_{t=0}^1 | X_i, D = 1] - E[Y_{t=1}^0 - Y_{t=0}^0 | X_i, D = 0].$$

The ATT^{DID} can then be estimated as

$$\widehat{ATT}^{DID} = (\bar{Y}_{t=1}^1 - \bar{Y}_{t=0}^1) - (\bar{Y}_{t=1}^0 - \bar{Y}_{t=0}^0) = \Delta\bar{Y}^1 - \Delta\bar{Y}^0$$

The double differencing removes the bias that results from a common time trend unrelated to the intervention as well as from permanent differences between the groups (Imbens & Wooldridge, 2009).

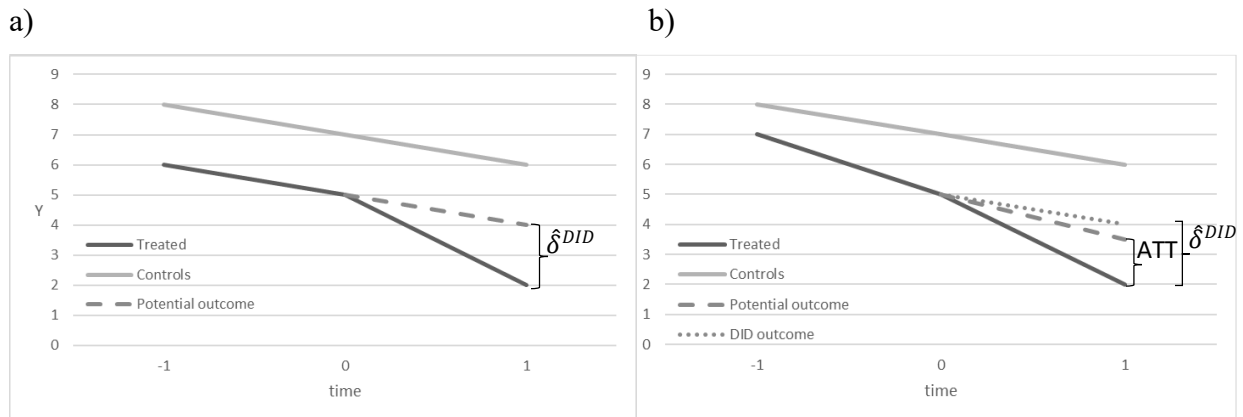
For the two-wave panel case, efficient and consistent estimation of the ATT if the only difference in the expected trajectories of Y^0 for both groups is in their levels (their intercepts) is possible by estimating $\hat{\delta}^{DID}$ in a linear parametric model

$$\Delta Y_{it} = \alpha_t + \delta^{DID} D_{it} + \Delta X_{it} + \Delta E_i + \Delta \varepsilon_{it}$$

(Imbens & Wooldridge, 2009). First differencing removes the unobserved individual fixed effects E_i . The first-difference-model assumes that in the absence of the treatment, any difference between mean outcome paths for those in the treated and the control groups remains constant over time.³ So if the effect of E_i on the outcome is time invariant, PTA will hold and $\hat{\delta}^{DID}$ will be an unbiased estimator for the ATT. Including covariates results in a conditional PTA assumption, meaning a shift of the outcome path is parallel for farms with similar covariate values.

The conditional PTA implies that selection into treatment depends exclusively on an individual fixed effect (i.e. permanent differences between the groups), which enters the model additively and linearly. That is, for the PTA to hold, unobservable characteristics must be time invariant so that they lead to a parallel shift in the outcome variable (Figure 3a). However, if environmental preferences or likewise unobserved factors affect treatment selection and are correlated with the outcome, self-selection becomes endogenous, which results in a violation of the PTA – analogous to the situation in Figure 1d). In this case $\hat{\delta}^{DID}$ will overestimate the true ATT because participants, if they had not participated, would have reduced the harmful environmental outcome more than the non-participants (Figure 3b).

³ The first-difference-model is also referred to as change score analysis. An alternative modelling strategy is to regress the outcome on a time dummy and an interaction of the treatment indicator with the time dummy. This is referred to as analysis of covariance. Which approach to favor depends on the underlying causal-effect paths; for example in case of selection based on the pre-treatment dependent variable, the analysis of covariance is preferable but in case of selection on fixed characteristics, the change score analysis is preferable (Allison 1990).

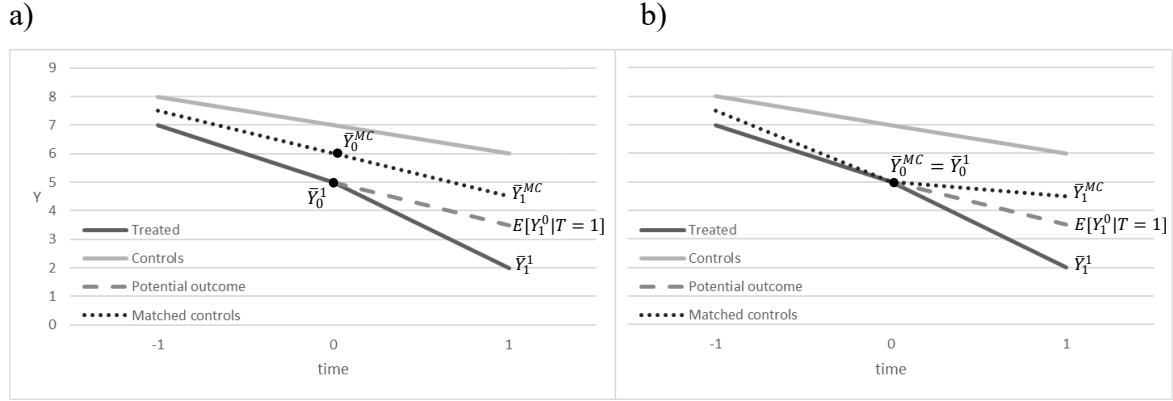
Figure 3: Average treatment effect from DID

Through the assumption of parallel trends, the first-difference model is consistent for cases a) to c) in Figure 1 – as is matching on \mathbf{X} the pre-treatment outcome. It cannot include effect paths of Figure 1e) because first differencing implicitly controls for the change in \mathbf{X} through including $\Delta\mathbf{X}_{it}$ in the linear parametric model. Here, the post-treatment farm characteristics are a mediating factor between treatment status and outcome, and part of the change in \mathbf{X} over time is caused by the treatment itself and controlling for the change in \mathbf{X} would lead to overcontrol bias, hence underestimation the causal effect.

4.3 DID-Matching

DID-Matching (Abadie, 2005; Heckman et al., 1997; Heckman, Ichimura, Smith, & Todd, 1998; Smith & Todd, 2005) allows for temporally invariant differences in outcomes between participants and nonparticipants, that is, an unobserved fixed effect. It is analogous to the standard DID regression estimator, without requiring the linear functional form restriction but still requires the parallel trends assumption (Smith & Todd, 2005). Thus, DID-Matching allows for time variant observed effects and time invariant unobserved effects.

For DID-Matching, a matched control sample instead of the whole control sample is used to estimate the treatment effect via double differencing. The matched control group is similar to the treated group in terms of observed pre-treatment characteristics, which makes the PTA more plausible. Consequently, the matched controls (MCs) are a better substitute for the potential outcome trend than the whole sample (Figure 4a). \bar{Y}_0^{MC} and \bar{Y}_1^{MC} represent the mean outcomes of the matched controls before and after the treatment period. \bar{Y}_0^1 and \bar{Y}_1^1 represent the mean outcomes the treated farms before and after AES adoption and $E[Y_1^0|T = 1]$ represents the potential outcome trend of the treated. If matching includes all relevant characteristics, the treated and matched control units will only differ in their baseline difference in pre-treatment outcome levels. The causal effect of participation is then identified by substituting the outcome trend of the matched controls ($\bar{Y}_0^{MC} - \bar{Y}_1^{MC}$) for the unobserved potential outcome of the treated.

Figure 4: DID-Matching (a) and DID-Matching on the pre-treatment outcome (b)


Many studies condition on the pre-treatment outcome, though without theoretical justification (Arata & Sckokai, 2016; Pufahl & Weiss, 2009). Chabé-Ferret (2017) shows that there is no situation in which DID with conditioning on the pre-treatment outcome is consistent when fixed and transitory confounders determine selection.

Matching on the pre-treatment outcome results in covariate balance ($\bar{Y}_0^1 = \bar{Y}_0^{MC}$) between treatment and matched controls in the pre-treatment period (Figure 4b).

The matching estimator is

$$\widehat{ATT}^M = \bar{Y}_1^1 - \bar{Y}_1^{MC},$$

and the DID-Matching estimator is

$$\widehat{ATT}^{M-DID} = (\bar{Y}_1^1 - \bar{Y}_0^1) - (\bar{Y}_1^{MC} - \bar{Y}_0^{MC})$$

When matching on the pre-treatment outcome, \bar{Y}_0^{MC} equals \bar{Y}_0^1 and \widehat{ATT}^{M-DID} becomes equivalent to the matching estimator:

$$\widehat{ATT}^{M-DID} = (\bar{Y}_1^1 - \bar{Y}_0^1) - (\bar{Y}_1^{MC} - \bar{Y}_0^1) = \bar{Y}_1^1 - \bar{Y}_1^{MC} = \widehat{ATT}^m.$$

If matching on the pre-treatment outcome is consistent (inconsistent) then DID-Matching will as well be consistent (inconsistent), thus superfluous (Chabé-Ferret, 2014). Therefore, DID-Matching on the pre-treatment outcome inherits only the properties of matching and not the strengths of both approaches.

Thus, DID-Matching on the pre-treatment outcome can be only consistent in the absence of an unobserved fixed confounder, which makes matching suffice. Worse, it is additionally likely to introduce bias compared to DID-Matching that does not condition on the pre-treatment outcome: in this case, the matched control sample would satisfy the PTA, but conditioning on the outcome forces pre-treatment outcomes to be equal on average (\bar{Y}_0^1). This restricts the

possible matched control sample to those with an outcome level lower than the average matched control sample (which makes DID after matching irrelevant). Unless there is a strong theoretical reason for using this subsample to represent the potential outcome, it seems likely that conditioning on the pre-treatment outcome introduces a new source of bias. For example, if the pre-treatment outcome becomes balanced and farms do not participate although pre-requisites for participation could have easily been met, the unobserved factors causing $D=0$ might be associated with a flatter slope as these farms might converge to the long run average factor use intensity. In this case, matching and DID-matching on the pre-treatment outcome would overestimate the ATT while DID-matching without conditioning on the pre-treatment outcome would be unbiased.

In summary, if selection is determined by an unobserved fixed effect and time varying observables, DID-matching is likely to be the least biased estimator for the ATT. However, for DID-matching to be consistent, the PTA must still hold for the matched sample. This requires that all covariates determining the time trend are observed and become balanced after matching. If unobservables have a time variant effect on the outcome, DID-matching still cannot identify the causal effect (cf. Figure 1d). Unlike matching and first differencing, DID-matching can incorporate the case in Figure 1e as it does not condition on post-treatment characteristics that might be moderators on the path between treatment status and outcome. Given the assumed possible cause-effect-paths, we illustrate DID-matching.

5 Illustration: Evaluating AES in Germany

5.1 Data, sample and AES participation

For illustration of pro-environmental land use effects of AES programs, we use a large sample of farms in West Germany from the EU-FADN in the period 2000-2006.⁴ We proceed as follows: first, we discuss and identify variables \mathbf{X} to be included in the matching model. These variables are those that influence the participation decision and the outcome variable simultaneously (Caliendo & Kopeinig, 2008) and lie along backdoor paths that generate non-causal association between participation and outcome (Pearl, 2000). We test their influence using a standard Logit model. Then we perform DID-matching using the Mahalanobis distance based with various matching algorithms (nearest neighbor and kernel density matching with optimal bandwidth selection) since which estimator suits best in terms of variance and bias reduction depends on the data at hand (Caliendo & Kopeinig, 2008). We then compare achieved covariate balances.

⁴ European Commission, Community Committee for the Farm Accountancy Data Network (FADN), Farm Accountancy Data for Germany 1999-2006.

We exclude farms specialized in horticulture and permanent crops and organic farms. Organic farms have a close to 100% participation rate, and horticulture farms have a very low participation rate, which makes it unlikely to find good matches for these farms. Furthermore, we exclude vineyards because AES are not primarily targeting vineyards and the vineyard farms that have additional cropping or livestock activities probably undergo a different decision-making than the other farm types, which would result in matches that are different on unobservable covariates. We calculate the effect of participation after the usual contract duration of five years. To maximize the sample of treated farms, we employ data from the whole first budgetary period from 2000 to 2006, accounting for variable starting dates. This allows us to use observations from different cohorts of participating farms during the first period with a minimum participation length of five years.

Most of the participating farms in the first period enter the agri-environmental programs in the first years from 2000 to 2002 (see appendix Table AI). For the DID analysis, six years of observations are necessary: the pre-treatment year plus five years of participation. As controls, we use only farms that never participated in AES during this period, meaning that farms that switch treatment state are excluded from the analysis. Thus, three cohorts are included in the analysis, namely farms with observed pre-participation years 1999, 2000 or 2001 followed by five years of continuous participation or non-participation. This yields a sample of 931 treated and 1431 untreated farms that can be used for DID estimation.

The model for AES participation is based on the specifications of Pufahl and Weiss (2009) and Arata and Sckokai (2016), and includes all theoretically relevant predictors of participation identified by Zimmermann and Britz (2016). The latter find that participation in AES is based on how well the schemes requirements can be integrated into the production program of the farm. This leads to two groups of variables that affect the participation decision: the production portfolio represented by farm type, livestock densities and cropping shares, and the farm characteristics, such as size, share of rented land and grassland, region, capital intensity and productivity. As indicators for pro-environmental outcomes, we consider fertilizer and plant protection expenditures as well as grassland share.

Table I: Means of farm characteristics by treatment state and cohort

Participation	2000		Cohort 2001		2002	
	No	Yes	No	Yes	No	Yes
Outcomes (5 years difference)						
Diff. in fertilizer exp. (€/ha)	11.18	5.27	-0.76	-3.88	9.87	-5.87
Diff. in plant protection exp. (€/ha)	3.09	-3.86	-0.23	0.91	9.55	-1.83
Diff. in grassland share (%-points)	-0.01	0.00	-0.02	0.00	-0.02	0.00
Farm characteristics (pre-participation)						
Age of farmer	45	45	45	45	45	47
Land input (ha)	60	54	65	80	64	59
Share of grassland area	0.38	0.39	0.37	0.3	0.34	0.31
Share of rented land	0.50	0.57	0.51	0.63	0.52	0.58
Share of cereals area	0.32	0.34	0.34	0.43	0.36	0.38
Cattle (LU/ha)	1.11	0.94	1.08	0.68	1.07	0.97
Pigs and poultry (LU) per ha	1.01	0.65	1.00	0.76	1.08	0.90
Sales per hectare (1000€)	2.79	2.45	3.04	2.43	3.50	2.85
Revenue per working unit	91	64	99	88	102	85
Revenue per capital	0.51	0.29	0.61	0.49	0.47	0.30
Fixed capital per hectare (1000€)	13.32	13.85	13.41	10.99	13.64	13.36
Fertilizer expenditure per hectare	88	74	104	88	113	108
Plant protection expenditure per hectare	69	70	73	79	82	83
Direct payments crops per hectare	161	171	166	214	172	197
Direct payments livestock per hectare	30	17	50	33	81	67
LFA participation (0/1)=1	0.00	0.00	0.12	0.47	0.13	0.42
Farm type						
Crop	0.22	0.19	0.25	0.30	0.27	0.29
Livestock	0.50	0.46	0.48	0.32	0.48	0.44
Livestock crop mixed	0.28	0.34	0.27	0.37	0.26	0.27
Region						
South	0.06	0.80	0.06	0.25	0.09	0.58
West	0.18	0.17	0.21	0.63	0.21	0.25
North	0.76	0.03	0.73	0.12	0.70	0.17
Participation in AES bef. 2000	0.08	0.94	0.07	0.44	0.08	0.48
N	458	740	497	139	482	52

Source: FADN

The mean and standard deviation of the outcome indicators and the farm characteristics are presented in Table I by participating and non-participating farms and by the three cohorts. The first rows present the outcome variables in terms of five years difference. For example, in the 2000 cohort the difference in fertilizer expenditure for non-participants is 11.18 €/ha and for participants it is 5.27 €/ha, resulting in a naïve DID estimate of -5.91 €/ha. At a base value for the treated of 74.41 €/ha before treatment, this amounts to a decrease by 7.9% in fertilizer expenditure in the first cohort. There is considerable variation across cohorts as the 2001 and

2002 cohorts have naïve DID estimates of -3.5% and -14.5%. The variation is even larger for plant protection expenditures where these estimates vary from -13.7% to +1.5%.

Participating farms have a larger share of rented land, less livestock density, lower sales per hectare, lower work and capital productivity and less fertilizer expenditures. Therefore, AES farms are characterized by a lower production intensity as the non-AES farms. Additionally, AES participation occurs much more in the south than in the north and participating farms also receive least favored area (LFA) payments.

In order to test the relevance of the farm characteristics for the participation decision, we use a logit model (Table II). Associated with AES uptake are share of rented land, cattle density, farm type, region and, for those farms with AES uptake in 2000, participation in environmental programs before 2000.

Table II: Coefficients of logit model for AES uptake by participation cohort

AES Participation (0/1)	Entry year					
	2000		2001		2002	
Age of farmer	1.00	(0.01)	1.00	(0.00)	1.00	(0.01)
Land input (10 ha)	1.02	(0.02)	1.01	(0.01)	1.03*	(0.02)
Share of grassland area	0.68	(0.33)	1.43	(0.46)	0.61	(0.25)
Share of rented land	2.49***	(0.75)	1.62**	(0.31)	1.40	(0.38)
Share of cereals area	0.37*	(0.20)	0.99	(0.34)	0.16***	(0.07)
Cattle (LU/ha)	0.72**	(0.11)	0.73***	(0.08)	0.77**	(0.10)
Pigs and poultry (LU) per ha	1.15*	(0.09)	0.90***	(0.04)	1.02	(0.05)
Sales per hectare	0.99**	(0.01)	1.00	(0.00)	0.99	(0.01)
Revenue per working unit (10€)	1.00	(0.02)	1.00	(0.01)	0.99	(0.01)
Revenue per capital (10€)	0.89	(0.11)	0.89**	(0.05)	0.99	(0.03)
Fixed capital (100€/ha)	1.00	(0.00)	1.00	(0.00)	1.00*	(0.00)
Fertilizer expend. (10€/ha)	1.00	(0.01)	0.97***	(0.01)	0.99	(0.01)
Plant protection expend. (10€/ha)	0.99	(0.01)	1.01	(0.01)	1.00	(0.01)
Direct payments crops (10€/ha)	0.97***	(0.01)	1.03***	(0.01)	1.05***	(0.01)
Direct payments livestock (10€/ha)	0.97***	(0.01)	0.99	(0.00)	0.99*	(0.00)
LFA participation (0/1)=1	1.68***	(0.24)	1.06	(0.12)	1.16	(0.17)
Farm type (Base=Crop)						
Livestock	0.89	(0.23)	0.96	(0.18)	1.25	(0.27)
Livestock crop mixed	1.36	(0.26)	1.33**	(0.18)	1.55**	(0.28)
Region (Base=South)						
West	0.04***	(0.01)	0.84	(0.11)	0.72**	(0.12)
North	0.03***	(0.01)	0.18***	(0.03)	0.20***	(0.04)
Participation bef. 2000						
Yes	23.71***	(3.77)	1.26*	(0.15)	1.06	(0.17)
unknown	8.97***	(1.71)	1.37***	(0.16)	2.85***	(0.35)
Pseudo R^2	0.615		0.155		0.155	
Observations	4212		3212		2296	

Coefficients given as odds ratios, standard errors in parenthesis, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, cross-sectional data at year of participation

Source: FADN

5.2 Average treatment effects of AES participation

We calculate the DID-matching estimator using nearest neighbor matching on Mahalanobis distance and kernel matching (Heckman, Ichimura, Smith et al., 1998; Heckman, Ichimura, & Todd, 1998) with optimal bandwidth selection (Galdo et al., 2008; Huber et al., 2015; Loader, 1999). Exact matching is imposed on year, region and farm type: treated farms can have their first participation in any year and must be matched to a control farm in the same cohort. Region is an important predictor of participation and captures otherwise unobserved climatic and structural effects that can influence the outcome. Farm specialization also seems a very important predictor for participation and different farm types undergo different decision-making, therefore matching farm types seems important for deriving a causal effect. We also match exactly on the quartiles of cattle density as otherwise balance on cattle gets worse after matching.⁵ Furthermore, we do not match on the respective pre-treatment outcomes, which are fertilizer expenditures, plant protection expenditures and share of grassland, respectively.

We first calculate the average treatment effects for the treated for the outcome of fertilizer expenditure (Table III).⁶ We additionally provide bias-corrected matching estimators where the difference within the matched groups is regression-adjusted for the difference in covariate values (Abadie & Imbens, 2006, 2011). This amounts to understanding the forming of matched samples as a preparatory data processing for a more robust parametric analysis (Ho et al., 2007). The matching algorithms differ in the resulting number of observations that are used for the calculation. For example, the 1:1 NN Mahalanobis matching uses only 182 of 1411 possible control observations. Similarly, the kernel matching with optimized bandwidth selection based on cross validation with respect to the explanatory variables uses only 83 treated and 121 control observations. The other kernel matching methods use decisively more observations.

The nearest neighbor estimators that use a fixed number of matched controls (one and five) do not yield significant results. The kernel methods that do not excessively prune observations (quantile distribution method and weighted cross validation) yield insignificant estimates as well. When comparing covariate balance across matching variants (see details below), it stands out that the cross validation kernel method is the only method that balances the indicator of participation in environmental programs before 2000. As all other covariates have similar balance, it might be that the balance on this item, which is also a significant predictor of participation, causes the larger point estimator.

⁵ For the NN-matching on fixed number of matches, we match on the quartiles of cattle density instead but do not enforce exact matching as otherwise the minimum number of pairs for correct standard error estimation cannot be reached.

⁶ Kernel matching estimators were calculated with the Stata user written program `kmatch` (Jann, 2017).

Table III: DID-matching outcomes for fertilizer expenditures

					Regression-adjusted ^{e)}	
Matching estimator	Matched treated	Matched controls	ATT in €/ha (s.e.)	95% CI	ATT in €/ha (s.e.)	95% CI
NN mahalanobis matching						
1:1 NN mahalanobis matching	924	182	-0.43 (7.01) ^{c)}	[-14.3; 7.8]	-8.82 (7.11) ^{c)}	[-22.7; 5.1]
1:5 NN mahalanobis matching	923	450	-3.21 (5.66) ^{c)}	[-14.3; 7.8]	-10.92* (5.95) ^{c)}	[-22.5; 0.7]
Kernel matching bandwidth selection method						
R*quant-dist. Method ^{a)}	764	1198	-2.29 (5.36) ^{d)}	[-12.8; 8.2]	-14.72* (8.85) ^{d)}	[-32.1; 2.6]
Cross validation	83	121	-10.50 (8.70) ^{d)}	[-27.6; 6.5]	-14.37 (10.7) ^{d)}	[-35.3; 6.7]
Weighted cross validation ^{b)}	729	1069	-4.31 (5.00) ^{d)}	[-14.1; 5.5]	-12.45 (8.63) ^{d)}	[-29.4; 4.5]
N	924	1411			924	1411

Notes: exact matching on year, farm type, region and quartiles of cattle density, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

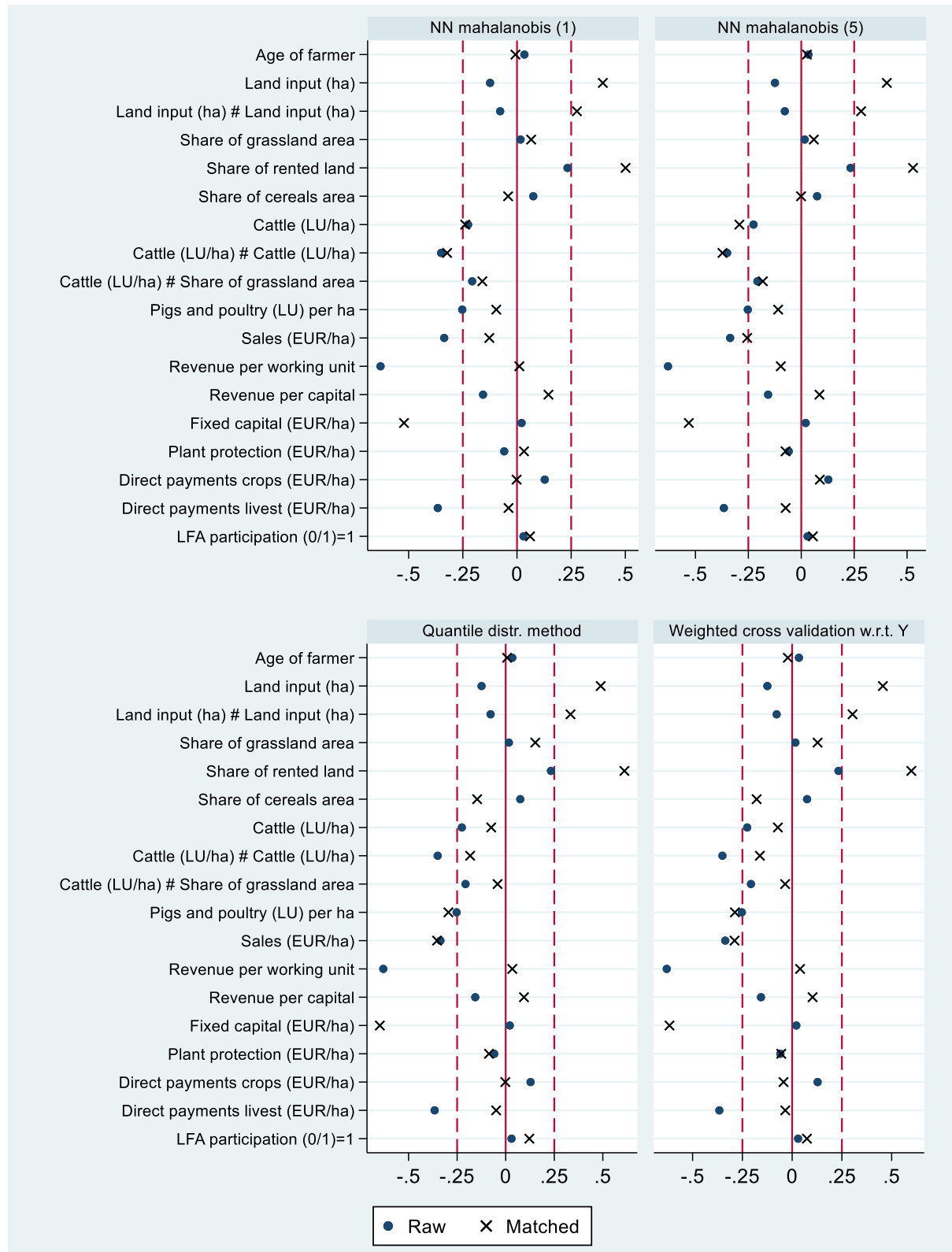
a) 1.5*90% quantile, Huber et al. 2015 b) Galdo et al. 2008 c) Abadie and Imbens (2008) standard errors

d) standard errors bootstrapped with 1000 replications e) Abadie and Imbens (2011)

As we cannot achieve balance on this predictor at reasonable sample sizes, we present regression adjusted estimators (Table III, right columns), for which remaining covariate imbalance within the matches is regression-adjusted for the difference in covariate values (Abadie & Imbens, 2011). The post matching regression adjustment suggest larger point estimators that are closer to the cross validation that achieves balances the pre-2000 environmental program participation. This suggests that balance in this covariate increases the impact of participation. However, only the quantile distribution method yields a weakly significant effect of -14.7 €/ha change in fertilizer expenditure. Given an average pre-participation expenditure of 78.4 €/ha this amounts to a reduction of about 19%.

To evaluate the matching procedures, we compare standardized bias before and after matching (Figure 5). Unlike t-tests, the standardized bias is not influenced by sample size, which can distort the results, as the pre-matching sample is usually much larger than the matched sample. Additionally the t-test does not reveal if balance on a confounder in fact increased after matching.⁷ Guidelines for what constitutes an acceptable standardized bias of a given covariate lie between 0.1 and 0.25, depending on the context (Harder, Stuart, & Anthony, 2010). For the farm characteristics in this study, a standardized difference of below 25% is considered acceptable.

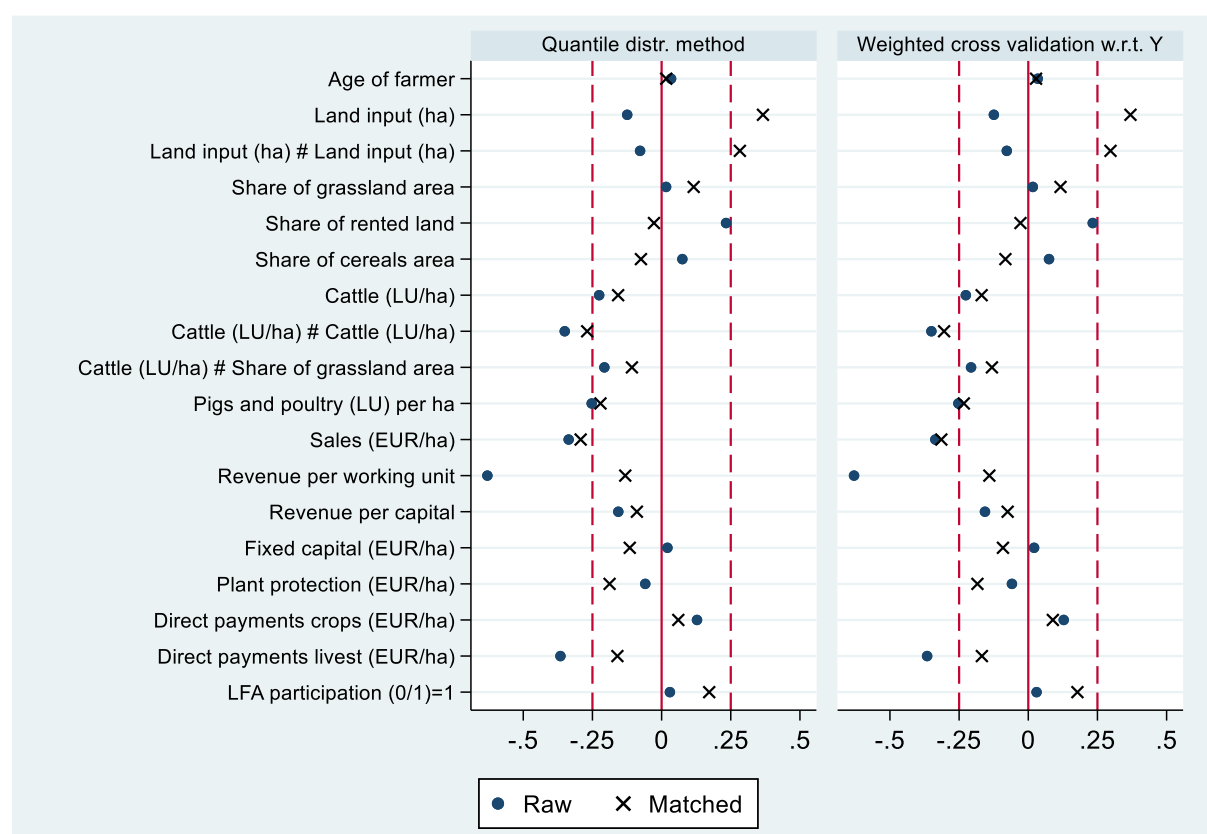
⁷ For more arguments against t-tests to assess balance before and after matching see Imai, King, and Stuart (2008).

Figure 5: Covariate balance before and after matching (outcome fertilizer expenditure)

Notes: matching on covariates listed, combined with exact matching on year, farm type, region and the quartiles of cattle density; imbalance in participation in environmental programmes not depicted, its standardized difference reduces from 2.4 to around 1.5 in these matching variants.

Covariate balance in terms of standardized bias generally improves after matching but remains larger than 25% for farm characteristics like size, share of rented land, fixed capital and pre-2000 experience with environmental programs. Although the matching estimates differ considerably in terms of used observations and significance levels of effects, achieved covariate balance is comparable. Therefore, we favor the kernel methods as they use observations more efficiently without increasing imbalance. The remaining imbalance on share of rented land seems problematic because share of rented land is an important predictor of participation (cf. Table II). Therefore, matching is repeated for the kernel methods forcing more balance on this covariate by exact matching on the quartiles of rented land shares. The exact matching on share of rented land matching improves balance drastically for both kernel matching variants (Figure 6).

Figure 6: Improved covariate balance (added exact matching on share of rented land)



Notes: matching on covariates listed, combined with exact matching on year, farm type, region and the quartiles of cattle density and share of rented land; imbalance in participation in environmental programmes not depicted, standardized difference reduces from 2.4 to around 1.5 in these matching variants.

The improvement in covariate bias decreases the number of treated units used for matching and the effect sizes increase (Table IV). The estimands from both kernel matching techniques become very similar and differ only in the second decimal place of the standard error. The regression adjusted estimands are significant ($p < 0.05$) and amount to a reduction of about 20%.

Next, we turn to the effect of AES participation on the change in share of grassland. Confidence intervals of the effect sizes in an increase in grassland range from zero to five percentage points. Covariate balance is similar to matching with regard to the outcome fertilizer expenditures (Figure A1). None of the matching algorithms yields significant effects for the reduction in plant protection expenditures.⁸ Therefore, we cannot reject that there is no impact of AES participation on plant protection measures.

Table IV: DID-matching outcomes for fertilizer expenditures, reduced imbalance

					Regression-adjusted ^{d)}	
Kernel matching	Matched treated	Matched controls	ATT (s.e.)	95% CI	ATT (s.e.)	95% CI
Outcome: Fertilizer expenditures ^{e)}						
R*quant-dist. Method ^{a)}	543	607	-5.93 (4.83) ^{c)}	[-15.4; 3.5]	-16.41 ^{**} (6.76) ^{c)}	[-29.7; -3.1]
Weighted cross validation ^{b)}	543	607	-5.93 (4.91) ^{c)}	[-15.5; 3.7]	-16.41 ^{**} (6.74) ^{c)}	[-29.6; -3.2]
Outcome: Change in share of grassland ^{f)}						
R*quant-dist. Method ^{a)}	543	611	0.03 ^{***} (0.009) ^{c)}	[.01; .05]	0.02 [*] (0.012) ^{c)}	[-.001; .05]
Weighted cross validation ^{b)}	557	625	0.03 ^{***} (0.007) ^{c)}	[.01; .04]	0.02 ^{**} (0.011) ^{c)}	[.001; .04]
N	924	1411			924	1411

Notes: exact matching on year, farm type, region, quartiles of cattle density and share of rented land, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ a) using 1.5*90% quantile, Huber et al. 2015 b) Galdo et al. 2008 c) standard errors bootstrapped with 1000 replications d) Abadie and Imbens (2011) e) outcome in €/ha f) outcome in percentage points;

5.3 Average treatment effects of AES participation by farm types

We investigate the AES impacts by farm type and consider specialized crop farms, livestock farms but also mixed farms defined based on the FADN typology (TF8). We cannot find any significant effects of AES on input intensity and grassland share for crop farms. For livestock farms, we find different effect patterns than in the pooled sample (cf. Table V). The effect on fertilizer expenditures is as expected negative but non-significant and the reduction in plant protection measures is significant. Changes in shares of grassland are with around five to six percentage points slightly larger than in the pooled sample.

Covariate balance measured in standardized bias lies within the bounds of 0.25 (for brevity see appendix Figure A2). This was achieved by additionally forcing exact matches on the deciles of the distribution of livestock density and quartiles of the rented land share. Covariate bias is

⁸ Results can be made available upon request.

better in the weighted cross validation. The reduction in plant protection expenditures of -9 €/ha can be related to average pre-treatment expenditure of 72.4 €/ha which results in a 12% reduction.

The analysis for mixed farms showed that matching did not result in acceptable covariate balance. Thus, treatment effects for the subgroup of mixed farms were not derived and we conclude that environmental impacts mainly originate from AES participation of livestock farms for changes in grassland share and reduction in plant protection measures. The fact that we do not find a reduction in fertilizer reduction in any of the subgroups but in the pooled sample might be due to sample size reduction in the subgroup specific analysis. Overall, the results for the livestock farms seem robust, as the covariate balance is much better than in pooled sample and the effects on plant protection and grassland share are larger and statistically significant, though sample size is about a third than that of the pooled sample.

Table V: DID-matching outcomes for livestock farms

Kernel matching	N matched				Regression-adjusted ^{c)}	
	Treated	Control	ATT (s.e.)	95% CI	ATT (s.e.)	95% CI
Outcome: Fertilizer expenditures ^{d)}						
R*quant-dist. Method ^{a)}	177	240	-10.3* (5.81)	[-21.7; 1.1]	-10.2 (8.27)	[-26.5; 5.9]
Weighted cross validation ^{b)}	169	204	-10.6* (6.13)	[-22.6; 1.4]	-11.1 (10.18)	[-31.1; 8.8]
Outcome: Plant protection measure expenditures ^{d)}						
R*quant-dist. Method ^{a)}	179	241	-6.8 (4.53)	[-15.7; 2.1]	-6.7* (3.88)	[-14.3; 0.9]
Weighted cross validation ^{b)}	170	205	-7.1 (4.9)	[-16.7; 2.5]	-9.0** (4.24)	[-17.3; -0.7]
Outcome: Change in share of grassland ^{e)}						
R*quant-dist. Method ^{a)}	178	242	0.03* (0.015)	[-.001; .06]	0.05** (0.021)	[.001; .09]
Weighted cross validation ^{b)}	168	197	0.03** (0.017)	[.01; .09]	0.06** (0.028)	[.001; .11]
N	411	691			411	691

Notes: exact matching on year, region, deciles of cattle density and quartiles of share of rented land, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, standard errors bootstrapped with 1000 replications a) using 1.5*90% quantile, Huber et al. 2015 b) Galdo et al. 2008 c) Abadie and Imbens (2011) d) outcome in €/ha e) outcome in percentage points;

6 Discussion, future research and policy implications

This paper was motivated by the fact that policy impact evaluation relies far too less on unbiased causal effect estimation. This is traced back to the two fundamental obstacles for causal impact evaluation: the non-experimental conditions and lack of data. We address both of these issues within our study. To approach the problem of deriving causal estimates from non-experimental situations, a natural starting point is to gain understanding of the self-selection mechanism of farmers into the programs. Thus, we base our analysis on alternative cause-and-effect paths. These offer the advantage of revealing where the knowledge base on how unobserved site-effects and unobservable farmer characteristics enter the impact pathway of AES participation on the environmental outcome is deficient. Furthermore, relying on cause-and-effect paths in causal inference enhances understanding about the validity of assumptions for causal effect identification and allows assessing the consequences of possible deviations from the theorized treatment assignment mechanism, which in turn fosters discussion about the plausibility of results.

Based on clarified assumptions for treatment effect identification, we illustrate DID-estimators combined with several matching algorithms including kernel matching with optimal bandwidth selection and post-matching regression adjustment. We estimate causal impacts of AES participation on sustainability of land use in West Germany. To be able to retain a sample size that is sufficient for DID-estimation, we used three cohorts of farms from the first implementation period of the new CAP period of 2000-2006. This is unique in the agricultural evaluation context and enables us to provide meaningful causal effects also by farm types. We match on Mahalanobis distances combined with exact matching on the pre-participation year. The outlined procedure serves as an illustrative case study for evaluation of AES and can easily be transferred to other European countries and programming periods in order to enhance understanding of the impact of agri-environmental measures.

We address the second major obstacle of policy impact analysis – lack of data – by showing that with a sophisticated data strategy, the FADN is in general a sufficiently large database, providing a representative set of panel observations that is large enough to perform this very data-demanding quasi-experimental analysis. To enhance understanding of the robustness of our results, we closely tracked standardized bias in covariates and sample sizes. This iterative procedure of assigning more importance to unbalanced covariates in order to force more balancing in this covariate at the cost of pruning observations worked well in this application. This, however, might be the exception rather than the rule. New procedures integrate these steps of bias minimization and sample size trade-off (King, Lucas, & Nielsen, 2017) and might offer valuable solutions for FADN-like datasets as well. Together with more suitable outcome indicators, such as nutrient balances, these methods seem promising to put causal agri-environmental policy evaluation into common practice.

Based on the explicit theoretical framework of cause and effect paths of AES participation and exploring its consequences for empirical analysis, we offer an informed understanding about future research needs: The cause-and-effect paths reveal the adoption decision as they point to

consider in the kind of relationships that form the basis of quasi-experimental impact evaluation. Past adoption studies employ a diversity of theoretical framings, which makes synthesis across cases difficult (Yoder et al., 2019). This limits the usefulness of the variety of studies to inform policy impact analysis. Facilitating future research, our findings highlight the role of environmental preferences of the farm manager but also risk behavior in decision-making, and how institutional factors such as social norms and rules influence the participation decision of farmers will be relevant. Closing this knowledge gap could yield important insights into how quasi-experimental methods within substantiated evaluation frameworks can improve agricultural policy analysis and thus design.

Our results also have relevance at the policy level: we provide tangible estimates for the contribution of CAP-measures to the environmental performance of the agricultural sector. Our results suggest positive environmental impacts of AES participation. Especially for livestock farms, we find a notable increase in grassland shares and reduction in plant protection expenditure. Thus, we have demonstrated that it is possible to assess the impact of environmental schemes and compare their efficacy within existing farm data networks. This could be of further relevance for the upcoming new delivery model for the post 2020 program period, which increases the necessity of large-scale impact evaluation. In order to make large-scale impact evaluation more meaningful, the FADN should be enhanced with more direct environmental indicators as the expenditures on fertilizers and plant protection are only very indirect environmental indicators. Combining FADN with IACS and/or with LPIS data offers promising future paths to improve the assessment environmental sustainability. In addition, information about AES should be more detailed. For example within the FADN, it is not possible to differentiate between AES measures. It could be helpful to analyze the impact of each measure and compare their efficacy to gain a better understanding of single AES measures.

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Appendix

Table AI: AES uptake by year in West German

Number of farms			Number of farms with six years of observations	
Year	Non-participants	Participants entering AES	Non-participants (no switchers)	Participants
1999	4,216	—	458	740
2000	2,769	1,482	491	139
2001	1,676	1,562	482	52
2002	1,643	677	—	—
2003	1,651	441	—	—
2004	1,567	397	—	—
2005	2,502	243	—	—
2006	2,636	414	—	—

Source: FADN

Figure A1: Covariate balance for outcome grassland share

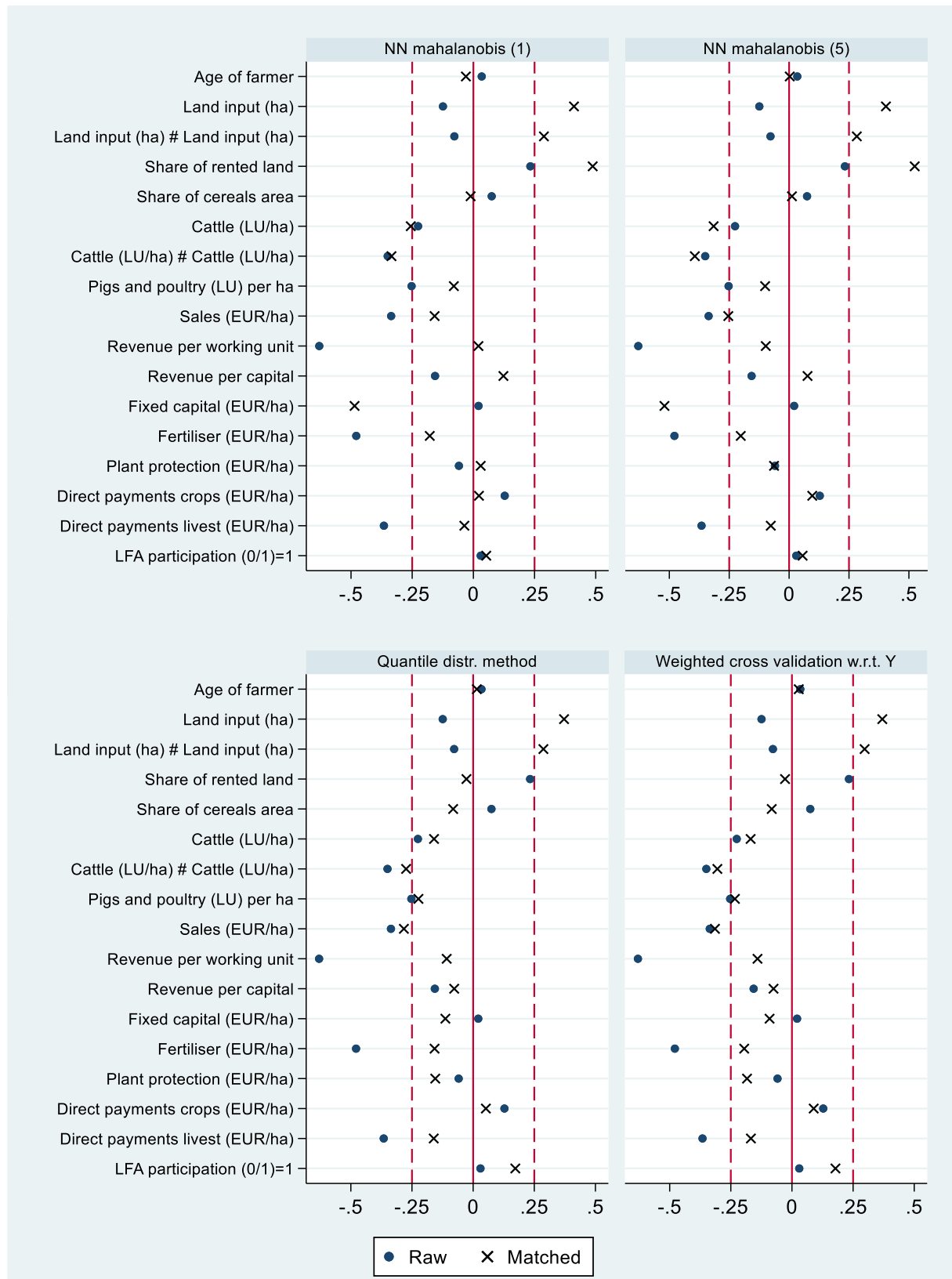
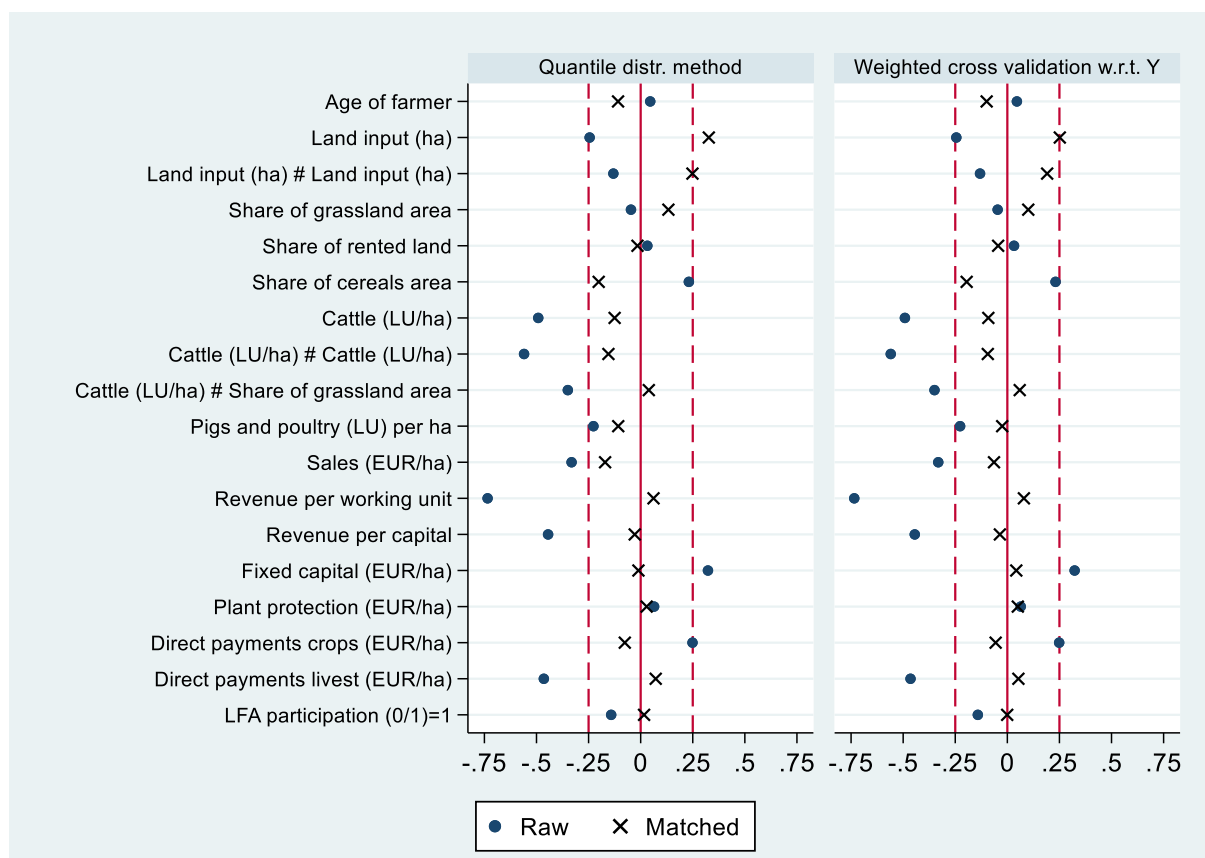


Figure A2: Covariate balance for livestock farms

Notes: matching on covariates listed, combined with exact matching on year, region, deciles of cattle density and quartiles of rented land share.

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